ON THE

DISCOVERY

OF THE

LACTEAL AND LYMPHATIC VESSELS.

Harvey, we are informed, discovered the circulation of the blood in the year 1616, although he did not publish his discovery until 1628. In the previous year (1627), Aselius, Professor of Anatomy at Pavia, discovered the lacteal vessels in the mesentery of brutes, and which had already been noticed in the writings of Hippocrates, Galen, &c., but without their attempting to give any description of the real nature or use of these vessels; consequently this subject remained in a state of darkness and doubt until the time of Aselius, who, on opening the abdomen of a dog, to observe the motions of the diaphragm, saw a number of white filaments on the mesentery, which he at first mistook for nerves, but on puncturing one of them, he found it to discharge a white milk-like fluid. He immediately and eagerly claimed the discovery of a new set of vessels, a fourth kind; for prior to this

time, the nerves were also considered as a system of vessels.

Aselius not only discovered the lacteal vessels, but also investigated and announced their peculiar office; namely, the absorption of chyle from the intestinal canal, and the conveyance of it into the blood.

For some time after the alleged discovery of these vessels, they were considered as fictitious by the majority of anatomists; and the doctrine which had been taught by Hippocrates, Galen, and other writers, was still believed, viz. that the mesenteric veins absorbed the chyle from the intestines, and conveyed it to *the liver*, where it was converted into *red blood*. Even for some time after the discovery of the lacteals in the human subject, in 1634, by Veslingius, and who attempted to give a drawing of them, a portion of the old system was retained; and it was affirmed, that the lacteals conveyed the fluid which they absorbed from the intestines into the liver, and that the chyle was there converted into red blood. In the year 1651, however, Pecquet, in dissecting a dog in order to observe the lacteals, discovered the lymphatics, and traced them to the thoracic duct: this, in point of fact, had been observed in the horse, by Eustachius, a Roman anatomist, in the year 1563, but from his not having any idea of the absorbent system, he called it "*vena alba thoracis*," and considered its function

to be that of nourishing the thorax. Some authors say that Olaus Rudbeche, a Swedish anatomist, first discovered the lymphatic vessels, while others maintain that it was Thomas Bartholine, a Dane.

We are told that Rudbeche appealed to his scholars and friends for his having a knowledge of the lymphatic vessels before Thomas Bartholine published his description of them, but that he never was able to persuade the majority of anatomists that the honour of the discovery did not belong to Bartholine.

Although we are informed that those anatomists were the first who discovered the lymphatic vessels, yet it appears they left this subject in a very unfinished and uninvestigated state, both as regards their *origin*, *distribution*, and *use*. Consequently the attention of anatomists and physiologists has been from that time to the present more or less excited, with a view of bringing this important subject to some acknowledged decision. Among the writers and experimentalists on this point, we have the names of Professors Boerhaave, Malpighe, Van Horn, Highmore, Ruysch, Nuck, Winslow, Cowper, Glisson, Lister, the Hunters, the Munroes, Cruickshank, Hewson, Mascagni, Sommering, Blumenbach, Magendie, Flandrin, Fodera, &c. &c. &c.

Opinions on the Nature and Use of the Lacteal and Lymphatic Vessels.

Boerhaave, as also his contemporaries and followers, considered the lymphatics as a system of serous veins ; either as taking their immediate origin from, or as being a continuation of, the minute terminations of the seriferous arteries ; and that their *use* was, to receive the *transparent*, and refuse the *red* or *coarser* part of the blood. This theory was more generally believed when the fact became known of Nuck, Cowper, Lister, &c. being enabled to inject the lymphatic vessels from the arterial. This continued to be a prevailing opinion until about the middle of the last century ; and although it was admitted that the lacteal vessels were for the purpose of *absorbing the chyle*, yet it was generally imagined, according to the old notion, that the *red* and *real* veins were the *principal absorbents of the body*. Still, although it is not generally noticed by authors, Boerhaave also taught that the lymphatic vessels were capable of absorbing fluids from the cavities of the interior of the body; his words are to the following effect :—" Hippocrates distinguished the solid parts of the human body into cavities and vessels ; the cavities in a healthy body, he says, are full of vapours, but in a diseased body full of sharp humours, or *ichor*. And it is certain that all the cavities and interstices in the

human body are supplied with a warm and moist vapour, which renders the membranes and muscular fibres pliable and fit for motion, and prevents them from adhering to each other. But this vapour is never discharged in such quantities as to turn into liquor, and prove offensive; for upon opening the *thorax* or *abdomen* of a living brute, nothing but a vapour exhales without any water remaining; *this vapour must therefore return again into the blood, which it can do by no other vessels that we are yet acquainted with, than the lymphatic veins.* Dogs have a communicative passage from their testicles into the cavity of their *abdomen;* which is not found in men. *Nuck* therefore wounded the *scrotum* of a dog, and injected a pound of water thereby into the cavity of the *abdomen*, sewing up the wound after the operation; the dog afterwards voided all the water by urine within the space of three days, so that no part thereof was found remaining in the *abdomen;* there must therefore be an open and continued passage from the cavity of the *abdomen* to the receptacle of the chyle. The warm and subtil vapour which is natural to the body, will be therefore much more easily admitted by the same vessels, though its quality be not inconsiderable; which is argued by the largeness of the cavities which are moistened therewith, as those of the *pelvis*, *scrotum*, *abdomen*, *thorax*, *pericardium*, *cranium*, *ventricles of the brain*, *cavity of the lungs*, *stomach*, *intestines*, *&c.* The same is also

argued from the sudden increase of a dropsy, where the vessels are not affected, but only the absorption of this vapour obstructed. As this vapour, therefore, appears to be so copious, it must have no small share with the other part of the lymph in assimilating the chyle, and rendering it the more easily convertible into blood and other juices proper to the human body."

These observations are taken from the second corrected edition of Professor Boerhaave's Lectures on the Theory and Practice of Physic, 1751. And sometime after this in the year 1762, it appears Dr. W. Hunter published his Medical Commentaries, in consequence of a controversy with Dr. Munro respecting some discoveries of the use of the lymphatic vessels. By his own imperfect reasoning, and some very rude experiments made by his brother, J. Hunter, he not only attempted to overthrow the doctrine of the lymphatics taking their origin from the arteries, but also endeavoured to prove that they were a *distinct and separate system of vessels, the sole absorbents of the body,* and that they took their origin from the external and internal surfaces of the body. This idea was founded on the known facts, that mercury, when applied to the skin, as well as collections of water, when formed in the abdomen, cellular membrane, &c. are occasionally taken up, conveyed into the circulation, and ultimately are passed off again by secretion: likewise from the great analogy between the lymphatics

and lacteals, both as regards their coats, valves, manner of ramifying, &c. ; and also from the lacteals being known and admitted to take their origin from the inner surface of the intestines, and being for the purpose of absorbing the chyle from those parts.

If Dr. W. and Mr. J. Hunter had merely confined themselves to the idea of the lymphatic vessels arising and absorbing from surfaces, &c. and then had given to them their proper share, as regards the *function of absorption*, these gentlemen, in my opinion, would have made a very important advancement in the science of physiology ; but they not only attempted to dispute their origin from the arteries, but also that the *red or real veins had nothing whatever to do with the process of absorption*. By this logic they endeavoured to make the veins appear almost of a similar nature with a set of *inert tubes*, viz. merely for the purpose of conveying back the red blood from the arterial system.

Dr. Hunter, prior to noticing Mr. J. Hunter's experiments in refutation of venous absorption, and wherein he attempts to establish *the whole process of absorption* by the lymphatics, remarks thus : —" With regard to absorption, I was of opinion that nature had provided a system on purpose, viz. the lymphatic. I considered these vessels and the lacteals as an appendage to the venal system, by which the stores were brought in for supplying

the circulation; and the glands and secretory vessels all over the body, I considered as an appendage to the arterial, by which the proper separations were made, and the redundancies thrown off.

"My only doubt was, whether the veins did or did not absorb a certain quantity, especially in the intestines. From my own observations on injections, I should have considered that they did not, and that there was no passage for liquors between an intestine and the mesenteric veins, otherwise than by transudition.* But authors of the best

* To more effectually do away with absorption by veins, it appears, that Dr. Hunter was under the necessity of denying that the mesenteric veins arose from the surface of the intestinal canal by numerous *radicles*; and which, before his time, as well as since, have been, by some of our best Physiologists, acknowledged as correct. From my own injections and experiments, I am ready to prove, and which is in accordance with the experiments of Ruysch, that the mesenteric veins, as well as arising from the arteries, also arise by numerous *radicles*, from the whole internal surface of the intestinal canal, which will be seen by injecting the trunks of the veins first, and the arteries afterwards, with yellow and red injection. The extreme *radicles*, or mouths of the veins, will appear almost as numerous as the terminations of the arteries. I am not of opinion that the mesenteric veins have *any share* in the absorption of the *chyle*, but I have not the least doubt, that they have a very great share in the absorption of the other fluids belonging to the intestinal canal. I also find (and which will be more fully explained hereafter) that the veins arise, by *open mouths*, from the *external surface of the skin*, viz. immediately beneath the *cuticle* or exterior covering of the true skin, almost, or quite as numerous as the arteries terminate; and a similar thing is to be seen on the external surface of the chorion, or that portion, which, in the living state, is attached to the cellular sub-

credit have given such arguments and experiments in favour of absorption by the veins, that I dared not, even in my own mind, determine the question. At this time, my brother was deeply engaged in physiological inquiries, in making experiments on living animals, and in prosecuting Comparative Anatomy, with great accuracy and application. It is well known that I speak of him in moderation, when I say so. He took the subject of absorption into his consideration, and from all his observations, was inclined to believe, that, in the human body, there was *one*, and *but one* system of vessels for absorption: he knew so well that many things have been asserted, by one person after another,

stance that forms the placenta, or bond of union between the mother and fœtus. To be convinced that the veins arise by *radicles* from the external surface of the skin, it may be proved, at any time, by injecting, with minute injection, the leg of a horse, cut off at the knee joint. If the valves of the leg vein are first of all broken down by the introduction of a whalebone probe, and the coloured injection be then introduced into the venal trunk, and the arterial one afterwards, on removing the hair, what I have already stated will appear very evident. Magendie observes, that "some organs appear almost entirely formed of venous radicles, such as the spleen, the *corpora cavernosa*, the *penis*, the *clitoris*, the *mammilla*, *the iris*, the *urethra*, the *glans penis*, &c. when an injection is thrown into one of the veins which proceed from the different tissues, they are completely filled with injected matter, the communications of the *cavernous tissues* of the penis with the veins, are made by openings of one-sixth to one-eighth of an inch in diameter."—*Page* 332, *Compendium of Physiology*.

which were not true, that so many mistakes had been made from inattention, so many errors introduced from other causes, that he could easily suppose, the veins might not absorb, after all the demonstrations that had been given of the fact, and, therefore, was determined to see how far this point could be cleared up by plain experiments and observations; with this intention he made the following experiments, in my presence, and in the presence of a number of gentlemen, who all of us assisted him, and made our observations on what passed before us. I shall quote the experiments from him, and can bear testimony to the fairness with which they were made, and with which they are here related."

MR. J. HUNTER'S EXPERIMENTS.

Animal First.

Experiment I.—On the third of November, 1758, in the presence of Drs. Clayton, Fordyce, Michaelson, and Messrs. Blount, Jones, Churchill and Richardson, says he, I opened the belly of a living dog. The intestines rushed out immediately, I exposed them fully; and we observed the lacteals filled with a white liquor, at the upper part of the gut and mesentery; but in those which came from the *ileon* and *colon* the liquor was transparent.

I tied up the mesenteric artery and vein that was going to about half a foot of intestine, and put a tight ligature upon the upper part of the intestine, including a little of the mesentery, then emptied that part of the gut, by squeezing it downwards, and put a similar ligature upon

the lower part of the gut. In the next place I made a small hole in the upper end of this part of the gut, and by a funnel poured in some warm milk, and confined it by making a third ligature upon the gut close to this hole. These ligatures prevented the circulation of blood in this part of the bowel. Lastly, I punctured the vein beyond the ligature that had been made upon the mesenteric vessels, and by gently stroking with the end of the finger soon emptied it of its blood.

Experiment II. I immediately after this made the same Experiment, and in the same manner, on a part of the intestine lower down, where the lacteals were filled with a transparent liquor.

In the first experiment the lacteals continued to be filled with a milky or white fluid: in the second, the lacteals, which before contained only a transparent lymph, were presently filled with white milk.

In both these experiments, we could not observe that the least white fluid had got into the veins. After attending to these appearances a little while, I put all the bowels into the *abdomen* for some time, that the natural absorption might be assisted by the natural warmth; then took out and examined attentively the two parts of the gut and mesentery upon which the experiments had been made: but the lacteals were still filled with milk, and there was not the least appearance of a white fluid in the veins; on the contrary, what little blood was in them, was just as thick, and as deep coloured as in the other veins, and when squeezed out from them, coagulated as the blood of other veins.

Experiment III.—I tied up and filled another piece of the intestine with milk in the same manner, but did not make a ligature upon the mesenteric vessels, leaving a free circulation in the part. We looked very attentively at the colour of the blood in the vein of that part, both

with our naked eyes and with glasses: we compared it with that in the artery, and in the neighbouring veins, but could not observe that it was lighter coloured, nor that it was milky, nor that there was any difference whatever.

Experiment IV.—Lastly, we took that part of the gut, which was filled with milk in the first or second experiment, and squeezed and pressed it very gradually, in order to see whether any milk would by these means pass into the empty mesenteric veins. This we did gradually with more and more force, till the gut at last burst: but still there was not the least appearance of any thing milky in the veins.

Animal Second.

Experiment I.—November 13th, 1758. In the presence of Drs. Wren, Fordyce, and Michaelson, Messrs. Blount, Tickell, Churchill, Paterson and Skeette, I opened the *abdomen* of a living sheep, which had eat nothing for some days, and upon exposing the intestines and mesentery, we observed the lacteals were visible, but contained only a transparent watery fluid. I made a hole in the intestine near the stomach, and by a funnel poured in some thin starch, coloured with indigo, so as to fill several convolutions: then tied up the hole in the gut, and put all the bowels into the *abdomen* for some time. Upon taking them out after this, we observed all the lacteals of that part filled with a fluid of a fine blue colour. We thought at first, that the blood in the veins of this part was of a darker colour; but upon comparing it carefully with that in the other veins it was manifestly the same.

Experiment II.—I opened a vein upon this part of the mesentery, and catched a table spoon full of its blood. I set it by to congeal and separate into its *coagulum* and *serum*. On the next day, and the day after that, I ex-

amined the colour of the *serum;* but it had not the least blueish cast.

Experiment III.—I fixed an injecting pipe in an artery of the mesentery, where the intestine was filled with blue starch, and tied up all communications both in the mesentery and intestine, *(as in animal first, experiment I.)* but left the corresponding vein free; then I injected warm milk by the artery, till it returned by the vein, and continued doing so till all the blood was washed away, and the vein returned a bright white milk. This was done with a view of seeing if the milk in the vein acquired any blueish cast; but there was no perceptible difference between the arterial and venal milk.

Experiment IV.—After this I opened the vein with a lancet, and discharged most of the milk, then put a ligature upon both the artery and vein, and waited some time to see if they would fill, but they did not, nor did the remaining contents of the vein acquire the least blueish cast. Then I opened the gut at this part, but we could not observe any appearance of the milk having got into the cavity of the intestine.

Experiment V.—I filled another part of the intestine with milk. All that we observed, after doing this, was that the lacteals became fuller, though not of a white colour, and the veins remained of the same complexion.

Experiment VI.—I fixed a pipe into a vein of the mesentery, and injected milk towards the intestine, to see if any would pass into the cavity of the gut: but presently innumerable extravasations happened; so that the experiment was fruitless.

Experiment VII.—I fixed a pipe into an artery, and tied up the vein, and all the communications, then injected milk for some time into the artery till the vein became quite turgid and tight; this was continued for some little time, and with as much force as we thought

the vessels would bear without bursting ; then we opened the intestine at that part, and there was no appearance of milk in its cavity.

Experiment VIII.—I took a piece of the intestine, that was quite empty and clean, and filled it with warm water. The returning blood in the vein of this part appeared not at all diluted or thinner than in the other veins. Then I tied up the artery, and all the communications, and attended to the state of the vein for some time: it did not grow more turgid, nor did its blood become more watery, nor was there any appearance whatever of the water having got into the veins.

The animal was quite alive all the time of our making these experiments and observations, which lasted from one o'clock till half an hour after three. I chose a sheep rather than a dog, both because the animal was much larger, and therefore its mesenteric vessels were fitter for being easily injected: and besides, because it is much more patient and quiet. These advantages we were all sensible of when we made the experiments.

Animal Third.

June 22nd, 1759. We repeated most of these experiments on another sheep, to see if the effect would be the same: but in this animal the viscera were diseased, inflamed and thickened in most parts, so that the experiments were much less successful, less satisfactory, and conclusive. After injecting milk into the mesenteric artery for some time, and allowing it to return by the vein, we opened that part of the intestine, which had been previously emptied, and found in it a watery fluid of a whitish cast, as if a few drops of milk had been mixed with it.

Animal Fourth.

In July, 1759. In the presence of Drs. Macauley, Ramsey, Michaelson, Messrs. Edwards and Tomlinson, I repeated most of the experiments related in article *Animal second,* upon another sheep. The effect of all of them was so nearly the same that I need not be particular.

I shall only observe, that when the intestine was filled with starch water and indigo, and milk injected by the artery till the vein was washed clean of blood, and a ligature put upon the artery and vein, so as to leave them about half full of pure white milk ; after waiting more than half an hour we could not observe that the vein was in the least more filled or turgid, nor had the milk in the veins acquired the least of a blueish cast, not even in the small veins upon the gut itself, where we should suppose the absorbed liquor must have been apparent, if any had been taken up by the veins from the cavity of the intestine.

After the animal was dead, I blowed into a mesenteric vein, and the air found a passage into the cavity of the gut, though in making the experiments when the animal was alive, I could not force the milk by injection, from the vein into the gut.

Animal Fifth.

If any animal could be supposed a fitter subject for such experiments than a sheep, it would be an ass, he is not so large, nor so strong, but that he may be managed; he is patient in the greatest degree; his mesentery and vessels being larger, it is so much more easy to fix injecting pipes, make ligatures, &c.; and, what is a very great advantage in making such experiments, his mesentery is very thin, without fat, so that the vessels are conspicuous

and distinct. Hence it is easy to separate the artery from the vein, to fix pipes, to tie up anatomising vessels by a needle, &c. Therefore I got an ass, and on the 24th of August, 1759, in the presence of Drs. Macauley, Michaelson, Messrs, Edwards, White and Gee, put him upon his back in an open garden, and tied him fast to four stakes driven into the ground, then opened his *abdomen, &c.*

Experiment I.—I poured a solution of milk in warm water into a piece of the intestine, and confined it there by two ligatures. In doing this the animal struggled, and a little of the liquor was spilt upon the outside of the intestine and mesentery.

After waiting a little while I opened with a lancet some lacteals of this part, which were full of a watery fluid, and catched a little of their contents in a small spoon. It smelled strongly of the musk; and though it could hardly be doubted that the musk had been taken up from the intestine by absorption, yet as some of the musk solution had been spilt upon the external surface of the parts, and as it was impossible to collect the lymph from the lacteals without resting the edge of the spoon upon the mesentery, the smell of the spoon might be owing to that circumstance.

After this I wiped a vein upon the mesentery very clean, and opened it with a lancet: a gentleman who had kept out of the way of the musk, came immediately with a clean spoon, and filled it from the stream of blood without touching any part of the animal, and carried it directly off: but it had not the least smell of musk.

Experiment II.—We poured some starch water, made very blue with indigo, into a part of the gut in the same manner as in some of the former experiments; tied the vein and artery of this part; then punctured the vein close to the ligature, and pressed out almost all the blood;

then tied up the empty vein, and put all in the cavity of the belly for a quarter of an hour. After that we examined the part, found the lymphatics very turgid, as the fluid could not pass through them towards the thoracic duct on account of the ligatures made upon the mesenteric vessels: but we found the veins of this part empty, except indeed that a little blood had got into them from the neighbouring vessels, which, from the appearance, had evidently passed the ligatures tied round the ends of the gut: a circumstance which it is very difficult to obviate.

Experiment III.—I next repeated the *third experiment* of *animal second* exactly in the same manner, and precisely with the same effect.

Experiment IV.—Then I repeated the *fourth experiment* of *animal second;* and the effect was still the same.

N. B. It may not be amiss to observe, that the lacteals continued to absorb the blueish liquor all this time; even at the part upon which this *fourth experiment* was made, where the nerves must necessarily have been tied up with the artery.

Experiment V.—I squeezed a piece of the intestine so as to empty it as entirely as well might be, then tied up all the lateral communications of the vessels, and injected warm milk into the mesenteric vein, till it returned by the artery, and continued this operation for some time after all the blood was washed out. Then I opened that part of the intestine through all its length, and found it quite empty. I made this experiment again upon another part of the intestine, in the same manner, and exactly with the same success.

Dr. Hunter, after introducing these experiments, then makes the following remarks:—"Here is a new doctrine proposed in physiology, viz., *that*

the red veins do not absorb in the human body. The fair inquirer after truth will be convinced, by the observations which occurred to me, that the common opinion is supported by some proofs that are at least doubtful or equivocal, and that the other opinion is not without plausibility; and he must allow that my brother's experiments render it highly probable."

At present I shall refrain from making any observations on these experiments, until I treat in separate articles on the nature and functions of the mesenteric veins, and the lacteal and lymphatic vessels. I shall only add, that from the time of the Doctor and Mr. J. Hunter to the present, more particularly in this country, it has been held and taught as a generally received opinion, that in the living body, there are three orders of vessels, viz., *arteries*, *veins*, and *absorbents*. And it has been again admitted, which I have every reason from my own injections and observations to believe correct, that the lymphatic vessels, as well as taking their origin from surfaces, &c. likewise arise from the extreme branches of the arterial system, as had previously been asserted by Nuck, Cowper, &c. The latter part of this statement Dr. Hunter and his early followers attempted to refute, as likewise that the mesenteric veins took their origin by open mouths from the inner surface of the intestinal canal. No wonder at this, because if these facts were to have

been acknowledged, they would always have stood as obstacles in the way of the whole of absorption being effected by the lacteal and lymphatic vessels, and also that the lymphatic was *only a part* of the true venous system, and under such circumstances could only perform its proportionate and proper share of absorbing faculty. To prove that the *lymphatic system* is only a part of the *true venous*, and BOTH SETS OF VESSELS, that is the red and the transparent or white veins *(lymphatics)*, absorb, and that *both* in short form but ONE *entire system of absorbent vessels*, (that these are *facts*, in the *strongest point* of view, and by *much stronger evidence* than all the *various experiments* which have been performed on these subjects,) is to shew that *a part* of the lymphatic system is at *certain times capable* of assuming the EXACT CHARACTER of the *true venous*. This I shall proceed to shew by a discovery which I made in the year 1826, and which has been published both in the Lancet and Veterinarian; I shall also shew by some cases that occurred in the College very shortly after I made this important discovery, that the lymphatic vessels, as well as taking their origin from surfaces, also arise from the extreme branches of the arterial system.

Discovery of the second Class of Lacteal Absorbents assuming a similar Character to the real Veins.

In the month of June, 1826 (see *Lancet*), I was requested by some of our pupils, among whom was Mr. Sible, who is now a veterinary practitioner at Norwich, to attend them to a slaughter-house near Smithfield, for the purpose of instructing them in the performance of some necessary operations, and also of demonstrating some of the most important parts of the interior of the horse. Accordingly we selected for examination a five-year old mare, which was brought to be destroyed on account of a disease in her foot. After she was struck on the head, and her throat cut, we proceeded to dissect the abdominal viscera, and, on detaching the cæcum from the colon, I was much surprised to observe the *second class* of lacteal vessels situated in the mesentery, and surrounding the lymphatic glands, among the adipose substance, near the spine, greatly enlarged, and containing dark red blood; while, on the contrary, all those lacteals of the *first class*, and which arise from the inner surface of the intestines, and terminate in the second, and the lymphatic glands, were much smaller in size than usual, and contained only a *transparent fluid*. The lymphatic glands, like the second class of lacteals, were full of dark red blood, at also the thoracic duct. I next examined the lymphatics of the lungs,

and other parts of the body, and found them in a perfectly natural state, except a few of them on the concave part of the left lobe of the liver, which appeared slightly enlarged, and containing a reddish fluid, but nothing in comparison with those already mentioned. Every organ of this animal appeared in a perfectly healthy state, except her foot, and which seemed to be incurably diseased. The stomach and intestines contained only a few ounces of food.

As I had never heard of nor seen any case of this kind before, I thought it best to have the parts carefully removed, and sent to the College for further inspection; accordingly a strong ligature was passed round the superior part of the mesentery, above the lymphatic glands, as near the spine as possible, to prevent the *dark red fluid* from escaping: these parts were then removed and sent away to be examined, which was done in the lecture room.

At various times during the day, and for some time afterwards, as long as these parts remained in a fit state, I repeatedly examined them, both by minute injections and dissections; and at last became convinced that this singular appearance did not arise from any disease, but that it was in consequence of *a natural or gradual enlargement of the second class of lacteal vessels*, whereby they were enabled to receive a large portion of *red blood* from

the extreme branches of the arteries which are given off from the trunks of the mesenterics, and which are for the purpose of supporting the lymphatic glands; and that this arose in consequence of the *function of digestion* being *suspended*, and the first class of lacteals ceasing to absorb any chyle, whereby the *second class of lacteals* became *increased in power*, and were thus enabled to absorb the surrounding fat of the mesentery, to assist in the support of the animal's life. I have proved this idea to be correct from repeated examinations of different cases since, and which the following one will serve to substantiate.

A horse was destroyed at the slaughter-house in consequence of having broken knees. Both joints were laid open. Previous to this it was reputed to have been very fat, but had, in consequence of suffering severe pain from this accident, fallen away very rapidly in flesh. On examining the abdomen there was a great quantity of fat on the mesentery, and the lacteal vessels nearest the intestines appeared in their natural state, with the exception of not containing chyle; while, on the contrary, those lacteals between the lymphatic glands and spine were much larger than usual, and, as in the former case with the lymphatic glands and thoracic duct, contained dark red blood. The arteries which supply these glands, and which arise from the mesenteric, were also much larger in size than

usual, as well as their corresponding veins. All the lymphatics in other parts of the body were in a natural state; all the organs healthy; and the stomach and intestines contained only a very small portion of food.

These parts, as in the former instance, were carefully removed and sent to the College for inspection; as also numerous others, which occurred under various circumstances hereafter to be mentioned, some of which underwent the inspection of Mr. Coleman, Sir A. Cooper, Sir C. Bell, Mr. Brodie, Drs. Barry, Pearson, &c.

For several years after the discovery of this curious phenomenon, as opportunities offered, I diligently attended the different slaughter-houses, and soon found that the fact, as regards the second class of lacteals assuming the same character as the *real veins*, not only occurred in horses suffering from pain, but likewise under a variety of other circumstances; such as when they were brought in to be destroyed in moderate condition, and in consequence of there being a great number of dead ones already on hand, they were obliged to remain for some time before they were destroyed, and during that period were only allowed a very limited portion of food, and thus fell away rapidly in flesh. When they were destroyed, I had a fair opportunity of examining a variety of specimens, wherein the second class of lacteals had assumed the same

character as the *red veins;* and I noticed that this occurred much more frequently in *very wet and cold weather than otherwise;* and was most distinctly seen in those horses which, a short time before they were destroyed, had been in a moderate state of condition. I likewise observed that this singular appearance of the lacteal vessels varied very much, according to the quantity of fat found in the mesentery. In those that were in moderate condition, and where there was a large portion of adipose substance, and in which the absorption appeared to have been going on in a very rapid manner, the second class of lacteals appeared of a similar character to those which I have already treated of, and were seen as *large red veins* arising from the lymphatic glands, and not only *ramifying around, but, for a considerable distance down the mesentery, then turning suddenly upon themselves, and proceeding in a straight direction toward the spine,* to terminate ultimately in the receptaculum chyli and thoracic duct.*

In others, when destroyed under similar circumstances to the former, but in a much lower and weaker state of condition, with only a very limited portion of fat in the mesentery, the second class of lacteals, &c. presented a similar appearance to

* I have now in my possession some drawings which were taken at the time, by a very accurate anatomical drawer and engraver, Mr. J. Stewart.

what has been stated, with the exception of the lacteal vessels and lymphatic glands not being, by a considerable degree, so large in size, nor the fluid contained within them of so red a colour.

From what has already been stated, it is evident that the lacteals, as they are termed, are also a *part of the true venous system;* and that they perform, when requisite, other important offices in the animal economy besides that of absorbing chyle from the surface of the intestinal canal. In short, it has been proved, by minute injections, that they also arise from the transparent or most minute branches of the mesenteric arteries which ramify in the peritoneal covering of the intestines, and, no doubt, from its surface also; and, if such be the case, they not only perform the office of absorbing a part of the peritoneal secretion, but also that of returning the white transparent or colourless blood from the seriferous arteries of all those parts to which they belong; and which is proved by the first class of lacteal vessels being found to contain a transparent fluid when the function of digestion is suspended, and there is no chyle for them to take up.

It, therefore, appears, that the lacteal absorbents are capable of performing four distinct functions; first, the absorption of chyle, or new white blood from the surface of the intestines; secondly, that of returning the white or colourless blood from the

seriferous arteries of the intestines; thirdly, that of absorbing a portion of the peritoneal secretion; and fourthly, as in very extraordinary cases of rapid absorption, in which the function of digestion is nearly or quite suspended, that of becoming considerably enlarged in size, by which they are enabled to receive a large portion of *red blood* from the arterial system, and thus proving them to be of a *similar nature to the real veins;* and not only this, but also proving, to a demonstration, that if, in cases of very rapid absorption in the mesentery, it be requisite for the second class of lacteal vessels to *assume the same character as the red veins, and absorb at the same time, the red veins not only are capable of removing, or of absorbing, but really do remove and absorb a much larger portion of the body* than *either the lacteals or lymphatics;* the latter of which, according to the theory of Mr. J. Hunter, have usually been considered as the sole absorbents of the body.

The Cases which occurred in the College, shewing the Communication of the Lymphatic Vessels with the Arterial; Nature, Use, &c.

CASE I.

A horse died of inflammation of the lungs. On examination after death, he was found to have a chronic disease of the liver. As the liver was removed, I was much struck with its great increase in size; and while

examining more particularly into this, I was still more surprised at finding all the lymphatic vessels, belonging to the peritoneal covering of the liver *greatly enlarged*, and containing *dark red blood*. I next examined the thoracic duct, and found it full of dark red blood, which had coagulated firmly in many places. The lymphatics in all the other parts of the body were of their usual appearance.

CASE II.

A glandered horse was destroyed. On our examining into the state of the lungs, &c. after death, a great number of tubercles were found in many parts of them: and on our minutely examining into the state of the parts diseased, all the lymphatic vessels which arose from them were much larger in size than common, and contained red blood; while, on the contrary, in all those parts of the lungs which were healthy, the lymphatics presented their usual transparent appearance. I next examined the contents of the abdomen, and found the left lobe of the liver slightly diseased, and the lymphatics from it filled with red blood; the right lobe was healthy, and the lymphatics from it in their natural state. The fluid in the thoracic duct was almost as red as venous blood.

Since I first noticed these facts I have been at the different slaughter-houses, and observed a variety of others of a similar description, in short, it is so common and may be seen so frequently, that no true inquirer will, for one moment, lose the opportunity of satisfying himself, on such an important physiological point.

At the time when these cases fell under my observation, I was not aware that it had already been recorded as a fact, that the lymphatic vessels were

capable of receiving red blood from the extreme branches of the arterial ones; but, soon afterwards, as I was perusing Magendie's Physiology, I found, to my great surprise, that this fact was there recorded in the following words :—"In animals dead in consequence of pulmonary or abdominal hemorrhage, Mascagni found the lymphatics of the lungs and of the peritoneum gorged with blood. He concluded from this, that these vessels had absorbed the fluid by which they were filled: but I have often found, both in animals and in man, lymphatics distended with blood, in cases in which there had been no effusion of that fluid; and besides, there is, in certain cases, so little difference between the *lymph* and the *blood*, that they cannot be easily distinguished." In another place he observes, "Whence, then, comes the fluid that is found in the lymphatic vessels? or, in other terms, what is the real or probable origin of the lymph? In considering, first, the nature of the lymph, which has the *greatest analogy with the blood;* secondly, the communication demonstrated by anatomy between the termination of the arteries and the radicles of the lymphatics; thirdly, the facility and quickness with which colouring and saline substances introduce themselves into the lymphatic vessels, it becomes, in my opinion, very probable, that the lymph is a part of the blood, which, in place of returning to the heart by the veins, follows the

course of the lymphatic vessels."* Magendie immediately remarks, that "this is by no means a new idea, but nearly the same as that of the anatomists who first discovered the lymphatic vessels, and who supposed that these vessels were intended to carry back to the heart a part of the serum of the blood."

Our countryman, the late Mr. Cruikshank, has also noticed the fact of the lymphatic vessels being frequently found to contain red blood in animals dying from inflammation of the lungs, peritoneal inflammation, strangulation, and other violent deaths; these vessels were discovered to be turgid with blood, without the least appearance of the extravasation of this fluid into the cellular membrane.

From the extracts which I have quoted from Magendie, it appears that this physiologist considers the red blood so often found in the lymphatic vessels to be transmitted from the minute terminations of the seriferous arteries into the extreme radicles of the lymphatics; and in this view of the subject I perfectly accord with him.

Having, I am afraid, departed a little too far from my subject, in relating what has already been advanced by authors, I shall return to what has occurred under my own observation.

From having at the time when the cases which I have first alluded to, (see pages 32, 33,)

* Magendie's Physiology, p. 317, 323.

were under consideration, an important subject already under investigation, viz. the varied changes which might occur in the second class of lacteal vessels, I was induced to attend also very minutely to the different changes which might take place in the lymphatics: accordingly, as before stated, I diligently visited the various slaughter-houses, and soon found that the fact, as regards the lymphatic vessels receiving red blood from the extreme branches of the arterial ones, was of much more frequent occurrence than I could have supposed; and I observed it to take place more particularly in those parts of the lymphatic system which were the *nearest to the centre of the circulation;* and, in every instance, the thoracic duct was found to contain a fluid of a similar nature.

What I have now stated occurred not only in those horses which were destroyed or died in consequence of chronic affections, but also in those that died from acute inflammatory diseases; and likewise in others that were destroyed while the *whole vascular system was in a state of high excitement, and in which there was not any particular internal disease present.*

I also found, that the red fluid in the lymphatic vessels and the thoracic duct varied its *red colour* precisely in proportion to the previous strength of the animal. In some very weak and debilitated subjects, and in which the circulation was quick

and weak, the lymphatic fluid was only *slightly tinged with red;* while, on the other hand, in all those in whom the circulation was strong and vigorous, the lymphatic fluid bore a character similar to that of the *dark red venous blood.* I also found the appearances already described, not only in the lymphatic system of horses which died while under a high degree of excitation, but likewise in many of those which were, sometime previous to death, *exposed to wet and cold,* and allowed only a very limited portion of food; and this existed *both with and without* the second class of lacteals containing any red fluid, and when the circulation was *diminished,* as well as when it was *increased.* I shall not now attempt to give an explanation of the nature and cause of this singular appearance of the lymphatic fluid, but shall defer it until I state my opinion with regard to the real nature and properties of the *blood, lymph, chyle, &c.* I shall, therefore, at present, only add, that Magendie has seen a similar occurrence in the lymphatic fluid of dogs, when destroyed after long fasting. His statement is as follows: "The lymph becomes redder in dogs, *according as abstinence is of longer continuance.* In some that had fasted eight days, I have seen it nearly of *the colour of blood.*"* Leonardo Franchini, in his numerous

* Magendie's Physiology, p. 325.

experiments in the lymphatic system, has also noticed a similar fact.

After having endeavoured to shew the immediate and intimate connection which exists between the lymphatic system and the arterial, as likewise some *very curious and ill understood changes* which are found to occur in the former, (hereafter more fully to be explained,) before I proceed further I shall in the next place revert to the opinions which have been entertained as regards their real nature and use.

It has already been stated, that the anatomists who first discovered the lacteal and lymphatic vessels generally conceived them to be only a set of serous veins, taking their immediate origin from, or being a continuation of, the minutest terminations of the serous arteries; that the *lacteals* arose also from the inner surface of the intestines; and that the use of these two orders of vessels was, for the purpose of absorbing the chyle, and also to receive the transparent, and refuse the red or *coarser* part of the blood;* and here they commonly conceived,

* "Every artery in the human body," says Boerhaave, "is larger than any branch that it emits, as we are assured by our senses, so long as the eye or microscope can trace them; and it is, doubtless, the same in those exceeding small arteries, whose minuteness and pellucidity conceal them from the eye, both naked and armed. But the particles of the contained fluid will be always in proportion to the diameter of their canals, so that if a small artery, admitting only

according to the old ideas, that all the other *absorptions* which were carried on in the body were

single red globules, is ramified, all its branches will be less than those globules, which they therefore will not receive, but they will admit those parts of the blood which are less than the red globules, or which are proportionable to their diameters, while the larger red particles will pass on into the red or sanguiferous vein. But the next lesser parts of the blood, to the red globules, are the yellow serous ones; and therefore the lateral branches of the smallest sanguiferous arteries will be filled with serous globules, and constitute a second order of vessels, viz. serous ones. That there are such serous vessels is proved by the microscope, injections, and the natural eye in an ophthalmia, where the red blood is forced into the serous vessels of the sclerotica. But these serous arteries again divide into smaller branches of less diameters than their yellow serous globules, and these branches will therefore be filled with the lymphatic globules which are the next less in size to the serous ones, and constitute a set of arteries of the third order, termed lymphatic arteries, such as furnish the aqueous humours of the eyes, which humours are absorbed or returned again to the blood by serous or lymphatic veins. Hence, then, the sanguiferous arteries will carry all the parts of the blood, the serous arteries will convey all but the red globules, and the lymphatics all but the red and yellow globules, &c.; and thus probably is the succession of vessels and humours continued, till the ultimate or last series of the smallest vessels convey only the most subtile juices of the body."—*Professor Boerhaave's Academical Lectures on the Theory and Practice of Physic*, vol. ii. p. 217, 1751.

I have introduced these remarks of Professor Boerhaave's, not because I agree with him as regards the *red and yellow globules, &c.* contained in the circulatory fluids, but because I accord with him as regards the ramification and distribution of the *sanguiferous* and *seriferous* arteries, veins, &c.

effected by the *red* or *real veins*. While, on the other hand, the next party, namely, the Hunters and their immediate followers, not only attempted to disprove that these vessels took their origin from the extreme branches of the arterial system, and that they were principally from surfaces, and *were a separate and distinct system of vessels, the sole absorbents of the body*.

Having proceeded thus far, they, in the next place, attempted to deny that the real veins had *any thing whatever to do with absorption*, although it had been repeatedly proved by Magendie, as well as a variety of other Physiologists, since that period, that the real veins actually *do perform the function of absorption*. But Magendie, as well as the followers of Mr. J. Hunter, have also, in my opinion, fallen into error,—1st. in doubting that the lymphatic vessels* are endowed with the faculty of absorbing;—2d. in considering that the process of absorption is almost a *mechanical process*;—3d. that the *arteries* possess the *power of absorbing* as well as the *veins*.

Mr. J. Hunter, who was the inquirer who theorized and claims the most as regards the functions of the absorbent vessels, informs us, that "the knowledge of the use of this system is but of late

* Magendie is still of opinion that the lacteal vessels are for the purpose of absorbing the chyle, but that this is the only office which they are capable of performing.

date, and the knowledge of its different modes of action is still later. Physiologists have laboured to account for its modes of action; and the *principle of capillary tubes** was, at first, the most general idea, because it was a familiar one. But this is too confined a principle for an animal machine, nor will it account for every kind of absorption. Capillary tubes can only *attract fluids;* but as these inquirers found that solids were often absorbed, such as schirrous tumours, coagulated blood, the earth of bones, &c. they were driven to the necessity of supposing a solvent. This may or may not be true: it is one of those hypotheses that can never be proved or disproved, and may for ever rest upon opinion. But my conception of this matter is, that nature leaves as little as possible to chance, and that the whole operation of absorption is performed by an *action in the mouths of the absorbents;* but, even under the idea of capillary tubes, physiologists were still obliged to have recourse to the action of those vessels to carry it along after it was absorbed, and might therefore as well have carried this *action* to the *mouths* of the *vessels*.

As we know nothing of the mode of the action of the mouths of those vessels, it is impossible we can

* This is in accordance with the newly-advanced theory of Magendie and Fodera, but which I consider to be equally as erroneous as that of the action of the mouths of the absorbents by Mr. Hunter and others.

form any opinion that can be relied upon; but as they are capable of absorbing substances in two different states, that of *solidity* and *fluidity*, it is reasonable to suppose that they have different modes of action; for, although any construction of parts that is capable of absorbing a *solid*, may also be such as is capable of absorbing a *fluid*, yet I can suppose a construction only capable of absorbing a fluid, and not at all fitted for absorbing a solid, though this is not likely; and, to see the propriety of this remark more forcibly, let us only consider the mouths of different animals, and I will venture to say, that the mouths of all the different animals have not a greater variety of substances to work upon than the absorbents have; and we may observe, that, with all the variety of mouths in different animals, this variety is only for the purpose of adapting them to absorb solids, which admit of greater variety in form, texture, &c. every one being capable of absorbing fluid matter, which admits of no variety."*

Mr. J. Hunter, after attributing such a wonderful power to the mouths of these absorbent vessels, and almost leading us to believe that they possessed a power of *breaking down* and *acting* on the more *solid parts* of the *living body*, in a similar manner to the *mouths* and *teeth* of *animals*, in the next place

* Hunter on the Blood, &c. vol. ii. p. 285, 286.

treats of "*ulcerative absorption, insterstitial absorption, progressive absorption, modelling absorption,* and of the *remote cause of the absorption of the animal itself;*" all of which I shall notice in their proper place, as also the manner in which I consider *absorption to take place.*

The immediate followers of Mr. J. Hunter have also gone so far as to *insist on teaching* us, that in *infancy* and *youth* the *arteries act as equal to two,* while the *absorbents act as equal only to one.* On the contrary, in *old age,* or the later periods of life, the *scale then becomes turned with these absorbents,* for they act as *equal to two,* while the *arterial system* then acts as *equal only to one.*

These opinions may appear very plausible to those who only take a superficial view of the subject of absorption; but, let me ask, if in infancy and youth the arteries act as equal *to two,* and the absorbents as equal only to *one,* where are those arteries which are considered as not to be capable of absorbing—where, I ask, are they to obtain the *materials* by which they are *gradually enlarging,* or *building* up the body? If the common labourers, the *absorbents,* act more slowly in conveying the materials to form and build up the *edifice* than the agents, or *arteries,* which are considered as the *active agents,* and who not only *commence, but complete the building,* where are they, if the absorbents act as already described, to obtain

their *materials?* All such questions as these will appear in their proper light hereafter; but, first of all, let us endeavour to come to a proper understanding as regards the conjoint *nature* and *functions* of the *real venous* and *absorbent system;* and as there are those who contend, that the *veins* are the proper *absorbents,* while, on the other hand, there are others who contend only for the *lymphatics,* let us, I say, out of these different opinions, and from what has come under our own observation, endeavour to form *one clear and indisputable system of absorption.* If, therefore, it can clearly be shewn that our first positions are *founded in truth,* then our reasoning, if properly conducted, will be *true also.* If, on the other hand, they are false, then all our reasoning, if ever so judiciously conducted, will, by some one or other, at some future period, be found to be erroneous also.

Remarks in Favour of Venous Absorption.

By the Hunterian experiments it will be understood, that the old doctrine of venous absorption was for some time almost entirely done away with, and scarcely supported by any one, until of late the French physiologists, and more particularly Magendie, have again drawn our attention to this subject; *but here they have fallen into a great error, in short equally as great as those who at first endeavoured to establish the doctrine of the whole of*

the absorption being effected by the lymphatic vessels. For in endeavouring to establish the ancient doctrine of venous absorption, those gentlemen at the same time, from not finding effects so evident in the lymphatic system as in the common venous, have endeavoured to do away entirely with the idea of the lymphatic vessels having *any share* whatever in absorption, and that their office is only to receive from the extreme branches of the arterial vessels the *transparent*, and refuse the red or coarser part of the blood, but in this idea they have fallen into equally as great an error as the former. For I shall hereafter shew that there is not the least doubt but that both the *realveins*, and *the lacteal* and *lymphatic vessels*, form but *one entire and perfect system of absorption.* Having made these remarks, I shall in the next place draw the reader's attention to some of the observations or experiments of Mr. J. Hunter, already inserted. Magendie states that Professor Flandrin, of the Veterinary School of Alfort, and himself, have repeated the experiments alluded to, and never in any instance did they find them to succeed, nevertheless he admits it as his opinion that the lacteals do absorb chyle, and that it is doubtful whether they absorb any thing else. Magendie's experiments on venous intestinal absorption appear to me to be very conclusive, some of them I shall hereafter introduce with several experiments and observations of my own. But in

the mean time it will be necessary for me to notice the remarks which have been made by Magendie on the experiments which were performed in the year 1758, by Mr. J. Hunter. "The following experiments were made by John Hunter, one of the first who positively denied the absorption by veins, and admitted that of the lymphatics; and it appeared to him to be very decisive. He opened the lower belly of a dog, he emptied several portions of the intestines of the matters they contained by compression, then immediately injected hot milk, which he retained by ligatures. The veins that belonged to these portions of the intestines were emptied of their blood by several punctures made in their trunks; he prevented the further introduction of blood by the applying of ligatures to their corresponding arteries, and he then replaced them in the lower belly, let them remain there for the space of half an hour, took them out again, and having examined them carefully, he found that the veins were nearly as empty as when he took them out the first time, they did not contain a drop of white fluid, whilst the lacteals were quite full." Magendie then adds,—"The imperfect state in which the art of making physiological experiments stood at the period in which J. Hunter performed his, can alone excuse this celebrated anatomist for not having felt how many important circumstances are wanting to give them weight, supposing them to

be correct. For, in order to have been of value, it would be necessary to know if the animal was fasting, if it was in the operation of digestion when opened; the state of the lymphatics ought to have been examined at the beginning of the experiment, as to whether they were or were not full of chyle. What changes happened to the milk during the time it was in the intestine, and what proof is there that the lacteals were filled with milk at the end of the experiment, was it not rather chyle with which they were filled."

Magendie again observes, "I took, says John Hunter, a portion of the intestine of a sheep, after having cut open the parietes of the abdomen. I tied it at the two extremities, and filled it with hot water; the blood that returned by the vein of this part did not appear more diluted or *lighter coloured* than that of the other veins. I then tied the artery, and all its communications. I examined the state of the vein; it did not swell; its blood did not become watery: thus it gave no indication of the presence of water in its cavity. The veins, therefore, do not absorb."

Then follows:—"How many objections present themselves when we wish for precision in experiments. How could J. Hunter judge from simple appearance; that in the first instant the water was absorbed, and did not mix with the blood in the vein. Then how could it be believed by this

author, who is in other respects so estimable, that the vein could continue its action when the artery was tied. He ought at first to have determined the effects of the ligature on an artery on the motion of the blood in the corresponding vein;—and this he did not do. In another experiment this Physiologist injected warm milk into a portion of the intestine; a few moments afterwards he opened the mesenteric vein, collected the blood which flowed from it, and, because he did not find any trace of milk, he concluded that there was no absorption of this liquid by the vein. But in the time of J. Hunter there was no means of ascertaining the existence of a small quantity of milk in a considerable quantity of blood; even at the present time, when animal chemistry is far advanced, this obstacle can scarcely be surmounted."

Magendie then adds: "These two experiments can have no effect upon the doctrine of venous absorption; the others, which are six in number, so far from being conclusive, are on the contrary more defective." In another place he remarks: "So great is the tendency of the human understanding to admit error: Hunter contrived a false theory upon one of the most important functions of life; he supported it with difficulty, upon some inaccurate experiments, which were every way insufficient. His ideas were immediately and universally admitted; *they are even defended at present*

with a heat and a zeal which truth but rarely inspires. Harvey, who instituted so many and such beautiful experiments to demonstrate the circulation of the blood, had to combat for thirty years, against the imputation of being a visionary, and to obtain a reception for one of the finest discoveries that ever did honour to human ingenuity."*

Some of our English physiologists, and more particularly Mr. Herbert Mayo, has of late discovered that the idea of Mr. J. Hunter, as regards the absorption of water coloured with indigo, was only an illusion,† for that in the ordinary state it is common to find the lacteals to assume a blueish tint after death. In this idea I perfectly accord with him, and more particularly when the animal has fasted a length of time before he is destroyed. But in no instance do we find this to be the case when an animal has taken food an hour or two previous, for then, on the contrary, we are in the habit of finding those vessels distended with a very large quantity of white chyle.

In the former instance this illusion may take place in consequence of the transparent nature of the fluid, while in the latter it is impossible,

* Magendie's Physiology, third edit. translated from the French, by E. Milligan, M. D. 1828.

† "When Mr. Hunter saw a white fluid rise in the lacteals after pouring milk into the bowel, we must suppose that some remains of chyle in the small intestine continued to be absorbed, and when the

from being then of much thicker consistency, and more opaque. At any rate it is of little account as to whether such be the case or not, for it is fully agreed on all sides, that the *lacteals absorb the chyle*, and therefore when this point is so very satisfactory, there is no necessity for any further experiments on this subject. The only thing therefore to determine is the *kind of absorption* which is effected by the mesenteric veins, and when this is once established a very important object will be gained. Dr. J. Elliotson, in his translation of Blumanbache's Physiology, 1820, after making some remarks on some experiments performed by Magendie, with the view

blue liquid was used, the deception probably resulted from the following circumstances: When the lacteals are empty, and are seen against a dusky medium, they appear as blue lines upon the mesentery. I have observed this circumstance when repeating the Hunterian experiment upon different animals. The lacteals, when a solution of starch and indigo was first placed in the cavity of the bowel, were full of chyle, on being examined half an hour afterwards appeared of a clear blue colour, and those present were for an instant satisfied that the indigo had been absorbed; but upon placing a sheet of white paper behind the mesentery, the blue tinge disappeared, the vessels were seen to be transparent and empty, on removing the white paper they assumed their blue colour.

"Thus the repetition of the Hunterian experiment has established a different conclusion to that which its author drew from it, and goes to prove the function of the lacteals to be limited to the absorption of chyle."—*Outlines of Human Physiology, by Herbert Mayo.* 3d Edit. 1833.

of proving that the veins absorb, says, " that he is ready to grant that Mr. Hunter's experiments deserve repetition, and the whole subject further investigation." But first of all let us, before we commence experimenting and unnecessarily destroying the lives of animals, let us, I say, come to a proper consideration of what we are going about, and as to what kind of vessels the lacteal and lymphatic absorbent vessels really are.

The Author's Opinion on the real Nature of the Absorbent Vessels.

On taking a survey of the creation, we find that there exists a variety of different animals, in which both *absorption* and *deposition* are going on, and yet throughout their circulatory system there is not the least particle of red blood to be seen. Animals of this kind are without a brain and spinal marrow, and are denominated *transparent or white-blooded:* they are, by comparative anatomists, divided into different classes, or great families, as the *mollusca articulata, &c.*

These are the varied classes of animals in which it is stated that the *lymphatic absorbent vessels* have not, as yet, been *discovered;* and yet physiologists are well aware that these animals possess a circulatory system of vessels, which, in many, the same as in the higher orders of animals, may be seen by the naked eye; and they are also well aware, that

there is *absorption* and *deposition* continually going on, yet they do not admit that there is a *separate system of vessels set apart for that sole purpose*, as in those of the higher orders of animals, or, in short, in those in which both *red* and *white* blood are known to circulate.

The fact is, Mr. Hunter's *theory*, as regards the *white or transparent veins*, more commonly called the lymphatic absorbents, from considering them as neither belonging to the *arterial* nor the *venous system*, but a distinct and separate system of vessels, viz. the sole absorbents of the body, and that the *red* or *real* veins had nothing to do with that function whatever, has, in my idea, been the means of leading them into error.

The reason of the lowest orders of the animal creation possessing so simple a system of vessels, appears to me very evident; namely, that these animals have their blood only of *one quality*, such as *transparent* or *white*; they therefore, require only such a system of vessels as are the best adapted for the circulation of transparent or white fluids, and these I should say, are a simple system of *transparent arteries* and *veins*, of the *same character as those vessels* which *belong* to all the *transparent* or *white parts* of such animals as possess blood of *different qualities*, such as *white* and *red*.

Hence, then, as in white-blooded animals, there is only one kind of blood, and one kind of blood

vessels, and those of the most simple form, such as *serous arteries and veins*, these *latter*, I contend, are similar in character, and of the *same nature*, as the transparent or *seriferous absorbent lymphatic veins*, which belong to all the different classes of animals of the higher orders of the creation, and which possess blood of different qualities, as *white* and *red*.

If from white-blooded animals we turn our attention to such as are called *red, or cold-blooded*, as *fishes* and *reptiles*,* we find that, although they possess red blood, yet the *greater portion* of their *bodies are supported* by blood of a *transparent or white colour;* blood of a *red colour* existing principally in those parts of the circulation nearest to the centre of the system, as the liver, heart, &c.

On the other hand, as we proceed to the more complicated part of the creation, such as are commonly called warm-blooded, as the mammalia and man, we find that they possess *red and white blood* likewise, but that the *red* parts of their bodies by far exceed those of the *white* ones. In the former instance, we include all the strong and more

* Mr Hewson was one of the first who investigated and described the lymphatic vessels in fishes and amphibious animals.—See *Philosophical Transactions*, 1769.

Hunter and Hewson were also the first who investigated the lymphatic vessels in birds.—See Hewson, *Philosophical Transaction*, 1768.

powerful parts of the body, as the *muscles*, *skin*, *mucous membranes*, some portions of the *larger bones*, *&c.* ; in short, all the various parts of the body which derive their support from blood of a *red colour* ; while, in the latter instance we include all the *white* parts of the body, which Galen and the ancients distinguished by the name of *spermatic*, as the *tendons*, *ligaments*, *cartilages* ; also the *serous*, *cellular*, and *synovial membranes* ; likewise the greatest portion of the osseous system ; in short, all the various parts of the body which are supported by *white* or *transparent blood*.

Most of these white parts just mentioned were considered as not possessing any blood vessels, and, of course, as not to be nourished by the blood, until, about the year 1700, Ruysch, the celebrated anatomist, first demonstrated, by his minute injections, that even the *compactest bones*, *cartilages*, *tendons*, *ligaments*, the *finest membranes*, *&c.*, are all furnished with arteries, in a similar manner to those of the red parts ; and, of course, that they derive their nourishment from, and are supported by, white or transparent blood.

Now, that we are fully aware that both the *white* and *red* parts of the living body are nourished and supported by blood, and that the blood in the *higher orders* of the animal creation is not only distributed to these different parts of a *red*, but also of a *white* colour, we must necessarily conclude

that this *transparent* or *white-coloured blood*, which is for the purpose of nourishing and supporting all the inferior parts of those *animals which possess also blood of a red colour*, is of a similar nature to the transparent or colourless blood, which belongs to, and supports, the *whole system* of all those animals that are called *white-blooded*, and are considered as the lowest order in the scale of the creation.

In both instances, that is, in the *entire system of white-blooded animals*, as also in the *white, or transparent parts* of such as are termed *red-blooded*, as there are transparent or colourless vessels adapted for the conveyance of nutritive fluids to and from these parts; and, as it is well understood, that in each, during the living state, there is absorption and deposition continually going on, therefore, although the Hunters might have carried their theories too far, in considering the white or lymphatic veins of red-blooded animals as a *sole and separate system* of absorbent vessels, yet, in justice to them, it must be confessed, that they were the first, in this country, who taught, in a general way, that these vessels possessed the faculty of absorbing from serous surfaces, as well as other parts of the body; for, prior to their time, these lymphatics were considered as having no share in the function of absorption whatever, as the physiologists of those days believed that the real veins were the sole absorbents of the body.

How the Hunters and their followers could have been led into such an error, in considering that the *red veins had no share* in absorption, and that the whole of the absorptions of the body were carried on by the lymphatics, I cannot possibly comprehend; and more particularly, when they were well aware that these *red veins* took their origin by open mouths, and, consequently, must absorb from the *cells of the spleen, corpora cavernosa, penis,* the *maternal portion of the placenta,* &c. Here are several parts from which they well knew that the veins took their origin, and from which they must absorb; and yet, in opposition *to these facts,* they grounded their theory on the following positions :—

" First, that mercury, when applied to the surface of the skin, was found to become absorbed; and that, consequently, this absorption must have taken place by the lymphatics, although they, at the same time, must have been fully aware that no one had ever discovered that the lymphatic vessels arose by open mouths from those parts." This is a position which I cannot grant them, but must give it in *favour of absorption* by the *extreme radicles* of the *real veins,* because I can prove, by minute injections, that these *red veins* take their *origin by open mouths* from the *external surface of the skin,* immediately under the *cuticle,* or the external covering of the *cutis,* or true skin,* and which has never

* See the note which gives an account of my injections.

been proved to be the case with the lymphatics, at least in the warm or higher order of animals. The idea, therefore, of cuticular absorption by the lymphatics must fall to the ground.

"Second, that collections of water, when formed in the abdomen, cellular membrane, &c. are occasionally taken up, conveyed into the circulation, and ultimately became passed off again by excretion." This is a position which I willingly give in favour of *absorption* by the *lymphatics;* first, because I cannot discover that the *red veins* take their origin by open *mouths* from the *serous* and *cellular* cavities of the body; secondly, because the lymphatic vessels are very numerous, and, as I consider, the *real white* and *absorbing veins belonging to those parts;* and, thirdly, in the natural state of the circulation, there are very few, if any, of the *red* veins to be observed in those white or transparent parts: while, on the contrary, the lymphatic veins are very numerous; and, although no one has ever been able to discover that they arise by open mouths from those parts (no doubt in consequence of their extreme minuteness), yet, still I consider them as the proper absorbents belonging to such parts.

"Third.—From the great analogy between the lymphatics and lacteals, both as regards their coats, valves, manner of ramifying; and also from the lacteals being known and admitted to take their origin from the inner surface of the intestines, and

being for the purpose of absorbing the chyle from these parts." All this I most freely admit in favour of *absorption* being carried on by the *lymphatic vessels;* and am only surprised that, at the present day, such a physiologist as Magendie, can for a moment doubt that "the process of *absorption is at all carried on* by the lymphatics;" and more particularly, when he must be fully aware that there is a very considerable portion of the living body denominated by the term *spermatic*, such as the *tendons, cartilages, ligaments, serous and cellular membranes*, &c.; and yet in these, like the red parts, there is both absorption and deposition always going on, and in the majority of them there is not the least particle of red blood, nor any trace of a real vein to be seen; but, on the contrary, there are found to proceed from all these parts numerous *white veins*, or lymphatic vessels. It appears, however, that Magendie is so far taken up with his theory of absorption by the *red veins*, as almost to believe, according to the *old idea*, that the lymphatics are only a system of serous veins, and that the simple office which these vessels perform, is to receive from the arteries, and return to the heart, the transparent blood from the extreme parts of the arterial system: thus leaving out of the question the idea that these vessels are also the *proper absorbing veins* belonging to the white parts, and which is further shewn from their intimate con-

nexion with, and the numerous ways in which these vessels proceed from, all the various serous and synovial cavities of the body, evidently shewing that they perform other important offices besides those stated by Magendie.

Magendie (in opposition to the third position of Mr. Hunter, wherein, from analogy, this physiologist attempted to prove that the lymphatic vessels really do absorb from serous surfaces, &c., in consequence of the lacteal vessels being a part of the lymphatic system, and it being known and admitted that they took their origin by open mouths, and absorbed the chyle from the inner surface of the intestinal canal) has stated that the lacteal vessels are incapable of taking up any thing else except the chyle from those parts. Now this, in my opinion, is proving nothing; for he might just as well have said, that the lymphatic vessels belonging to the serous membranes, such as the pleura and peritoneum, are incapable of taking up any thing except the serous fluid belonging to those parts.

Before, however, I proceed further, I shall notice some of the principal experiments which he has performed on this subject, and which, in my idea, prove nothing against lymphatic absorption; and only go to shew, that the real veins are capable of performing the faculty of absorption, and which I have not the least doubt of.

Further Remarks in favour of Venous Absorption.

Magendie informs us, that Dr. Segalas and himself took a portion of the intestine of a dog, and insulated it from the adjoining intestine by two incisions; that they left entire a mesenteric artery and vein, besides a number of the lacteal vessels; that in this dog they divided the vein, and by that means gave access to the venous blood, while, in another dog, they passed a ligature around the vein, so as to stop the current of venous blood; that they then introduced into each detached portion of intestine, at the same time making these parts secure by proper ligatures, a quantity of the watery solution of the alcoholic extract of nux vomica; they then returned each portion of intestine into the bellies of the dogs, and in the latter instance perceived no poisoning effect, after having waited the space of one hour. After this, they re-established the natural circulation, by untying the vein, and then the poisoning took place in the space of six minutes.

Magendie infers from these and some other experiments, which I shall notice hereafter, that the red veins are the real absorbents of the body, and that the lymphatic vessels do not absorb; and that it is very doubtful whether it is the office of the *lacteal* vessels to absorb any other fluid except that of the chyle from the inner surface of the intestinal

canal: forgetting, perhaps, that he has also informed us, that the function of absorption is only a mechanical one, and that he laboured for twenty years before he arrived at this fact, no one having ever dreamed of it before.*

Now, how Magendie can ridicule the idea that the mode of absorption is not a *vital phenomenon*, and also contend, that there is not the least difference between the *mode of absorption* in the various tissues of the body, either in the *living* or in the *dead state*, I cannot possibly comprehend; when, at the same time, this physiologist is under the necessity of admitting that the *lacteal absorbents* take up *with judgment* one substance, and at the same time *refuse another*. He says, that "extreme repugnance to confess our ignorance, and a tendency to admit romances, contrived to fill up the vacuities in science, are intellectual phenomena as remarkable as they are injurious to the progress of knowledge. It was not known how absorption was performed: instead of confessing as much, which might have proved a stimulus to new researches, some person thought proper to assert, *that the living tissues cannot admit of absorption as after death; that there are absorbing orifices which take up with judgment certain substances and refuse others.* This little story pleased many physiologists:

* Magendie Phy., p. 350.

they repeated it; *believed firmly in it;* and, from that time forth, none knew that the *mechanism of absorption* was still unknown, and, consequently, none ever thought of making it the object of research."

But let us return to the experiment: Magendie informs us, that the poisoning did not take place until after the ligature was removed from the vein, and the current of the circulation was again established. I suppose he means to say, that the effects of the poison were not capable of destroying life, and that, in removing the ligature, death from the absorption of the poison took place in six minutes.

Now my idea, in this instance, is, that the lacteal vessels really did absorb a portion of this poison, but that the absorption was *so slow,* in *comparison* with that of the large mesenteric veins, as not to be of sufficient effect immediately to destroy the life of the animal. For it is a well known fact, that if a *drachm of hydrocyanic acid* be introduced by a small piece of sponge into the cellular membrane of the extremity of a large ass, by making an incision through the skin, and if a ligature be applied, of a moderate degree of tightness, around the extremity, above the part where the opening is made, no poisoning whatever will take place, but absorption will gradually go on, and which is evinced by the animal becoming more dull and stupid than usual; and which, after a short time,

will gradually go off, and the ligature may then be removed without any evil consequences ensuing. While, on the contrary, if a similar quantity be introduced in the same way without a ligature being used, death, in the course of eight or ten minutes, will be the result: shewing, therefore, that it depends on the quick manner in which any poison is introduced into the circulation, and *no proof whatever against the lymphatic vessels performing the function of absorption* as well as the red veins.

Although Magendie admits that the lymphatic vessels are a system of serous veins, yet it is most surprising that he should doubt that they possess the faculty of absorbing; and this arises, no doubt, in consequence of his not having found the effects so certain, and follow so *rapidly*, as when absorption of any poison takes place in those parts, as the mucous membranes, &c., in which there are innumerable radicles of the real veins. Neither does he appear to take into consideration the extent of the surface necessary to produce a slow or a quick mode of absorption; nor whether there is any difference between absorption from a serous or a mucous surface, forgetting that, in the former, these membranes in the natural state of the circulation possess arteries and veins, which convey transparent or white blood only: while, in the latter instance, in the mucous membranes, the blood is of a different character, the circulation much more

strong, and, of course, when absorption takes place, as in the latter instance, it must be more effectual than in the former.

The Nature of Absorption as effected by the Mesenteric Veins.

That the extreme radicles of the mesenteric veins really do perform the function of absorption from the inner surface of the intestinal canal, in accordance with the opinion entertained of them previous to the time of the Hunters, cannot, in my idea, be the least doubted of; for if such was not the case, there would have been no necessity for nature to have caused these vessels to have taken their origin by open mouths from these parts, as we now at the present day know to be the case; which was also in former times fully believed, and as we are informed, were beautifully exhibited by the injections of that celebrated anatomist Ruysch, but which, in consequence of the experiments of Mr. J. Hunter, until of late, have been almost entirely lost sight of.

The following experiment, which was performed in the year 1826, will also go to support what has already been advanced by Magendie, and others, on this subject. Having kept a dog for some time without allowing him to take any kind of food or drink; immediately previous to performing the ex-

periment, I allowed him to take a pint of milk and water; then, after properly securing him, an opening was made into the trachea, through which was introduced a tube, which was connected with a small pair of bellows; and in the next place, the spinal marrow was pierced with a sharp-pointed probe, between the occipital bone and the first cervical vertebra, for the purpose of producing a partial insensibility. The bellows were then slowly worked by an assistant, with the view of producing respiration artificially; and the cavity of the abdomen was laid open to its whole extent, by means of which, the stomach, intestines, liver, &c. were freely exposed. Finding the muscular action of the stomach, as also the peristaltic motion of the intestines, and likewise the circulation of the blood still going on in these parts, in the next place, I divided the sternum; opened freely the cavity of the chest; exposed the heart, lungs, &c.; and then on the right side of the spine I divided the thoracic duct, from whence there flowed about a tablespoonful of white lymphatic fluid.

Having kept up respiration for the space of one hour and twenty-five minutes, I was then induced to see what effects would follow on dividing the hepatic duct at its entrance into the duodenum; as soon as this was done, there escaped from thence a small quantity of bile; and, very soon after this had taken place, the circulation of the blood

through the mesenteric arteries, veins, &c. became very greatly diminished, as also the peristaltic motion of the intestines, and the circulation of the blood through the heart, lungs, &c. ; and, in the further space of a few minutes, all these efforts of life entirely ceased.

Examination after Death.—On opening the stomach there were found only about two teaspoonsful of coagulated milk, and a small quantity of gastric juice. The duodenum, and other small intestines, were covered on their internal surfaces with a small quantity of mucus and biliary fluid, while the large intestines, as the colon and rectum, contained a very small portion of fæcal matter. I then collected together all the contents of the stomach, intestines, and the fluid which had escaped from the thoracic duct during this experiment, and found the quantity not more than one-third of a pint.

From this experiment, therefore, it becomes very evident that the intestinal absorption of the fluid must have taken place by the mesenteric veins to a much greater extent than by the lacteal absorbent vessels ; and, as a proof of the function of absorption and excretion having been carried on, it will also be proper to state, that the bladder became filled with urine during this experiment.

As a further proof that the real veins do absorb a large portion of the fluids which are taken into the stomach and intestinal canal, Magendie very

justly observes, "that there are some persons who drink from two to three gallons of mineral waters in a few hours, and reject them almost at the same instant by urine. Also, that it is impossible for the thoracic duct, from its smallness in size, to be enabled to give a sufficient passage to these and the lymphatic fluid at the same time." Boerhaave has also mentioned a similar fact, and he conceived, in such instances as these, absorption must have taken place by the veins of the stomach and intestinal canal.

We find, likewise, in our veterinary practice, that horses, when deprived of water for some time, will, if allowed, drink an enormous quantity at one time; almost the whole of which, instead of remaining in the stomach and small intestines, will in place thereof pass directly into the *cæcum* and *colon*, from whence it will quickly become absorbed, to be again passed off by excretion.*

Now, in such instances as these, it is very evi-

* Mr. Coleman relates an experiment in his lectures, wherein he kept a horse for a length of time without water, and then allowed him to drink a large quantity which had been previously coloured by the addition of a portion of ink; on destroying him, immediately afterwards, he found that the greater part of this water had passed direetly from the stomaeh, *through the small intestines*, into the *cæcum*. From the space of time in whieh this took place, Mr. C. supposes that the water must have travelled at the rate of *ten feet per minute*. This gentleman is also of opinion, that the veins absorb from the inner surfaee of the intestinal eanal.

dent that the *extreme radicles of the minute branches* of the large veins of the *cæcum* and *colon* must, under these circumstances, absorb the principal portion of the water admitted into these parts. Besides, the very great extent of surface for absorption which these two intestines present, as well as the innumerable ways in which the radicles of the veins arise from the inner surface of these parts, also in a very great measure tend to confirm that which has been already advanced on the subject.

It is, I say, very clear, from the great rapidity with which the water is absorbed, that this absorption must principally be effected by the extreme radicles of the mesenteric veins. Besides, the lacteals are not at all numerous in these parts; in short, not a quarter part so numerous as they are in the small intestines, as the *jejunum* and *ileum*, from whence by far the *greatest quantity* and the *best portion* of the chyle is absorbed, and in which there is always a very limited quantity of water to be found. Hence, then, the small intestines, as the *jejunum* and *ileum*, may be considered as those parts of the intestinal canal from which the more pure or better part of the *chyle*, *new* or *white blood*, is absorbed; while, on the contrary, the large intestines, as the *colon* and *cæcum*, may be considered as those parts from which the *more watery*, or by far the least nutritious parts are absorbed

or taken up; and there is not the least doubt in my mind that this latter function is effected by the minute radicles of the mesenteric veins to a much greater extent than we have hitherto been aware of.

If, therefore, not only veterinary, but likewise practitioners in human medicine, were to pay a little more attention to these subjects, and not to be led away with the idea that the lacteal vessels are the sole absorbents of the intestinal canal, and that the real veins have not any share whatever in that function, they would, in my opinion, be enabled *more clearly to account for the different action of purgative medicines*, as also, *the function and diseases of that very important organ, the liver:* as it is a well known fact, that if the mesenteric veins really do absorb fluids from the intestines, as already stated, these fluids before they can enter into the general circulation, to be passed off again, must of necessity first of all circulate with the venous blood through the liver. I have not the least doubt of this being the case; and more particularly, when it is found, that those persons who are known to drink very large quantities of spirituous liquors, either with or without being combined with water, are always much more subject to diseases of the liver than those who live otherwise.

If that which has been already announced is acknowledged to be generally correct, it will, also,

in a very great measure, tend to explain the *real nature and causes* of the diseases of the liver, &c. of some of our animals, and more particularly of *sheep*, which so frequently occur in wet seasons, as well as when they are kept in *very low wet situations*, more particularly in the autumn of the year, and commonly denominated *the rot*.

The food, under these circumstances, contains a *much larger portion of moist watery matter* than is really necessary for the healthy support of these animals, and the quantity of water imbibed with the food in such seasons being to a much greater extent than the more *solid nutritive matter* in the more dry seasons; the necessary consequence is, that a much less quantity of *real healthy chyle*, or new white blood, is then made than when the *food contains less moisture*, and, of course, *a greater quantity of more solid nutritious matter*. Hence, then, from the diminished quantity of pure chyle being made, the function of absorption by the lacteal vessels, no doubt, becomes much less than is natural, and the entire system very soon suffers from direct debility, the result, I should say, of *imperfect chylification*. While, in addition to this, from the very large or *unnecessary quantity of watery fluid* which in wet seasons is always taken in with food, the mesenteric veins, no doubt, for a time attempt to remove it by a process of absorption in the natural way: but in consequence of this undue quantity

of *watery fluid being compelled to pass from the mesenteric veins through the substance of the liver, a chronic inflammation* of that very important organ is the ultimate and not distant result; and hence follows dropsy, frequently diarrhœa, and at length, death.

In such cases as these, the biliary ducts after death are very frequently found to contain a great number of flat worms, commonly called *flukes*. These by many are considered, in addition to the wetness of the seasons, as the cause of the disease of the liver. I am not inclined to believe that they are of any service; but I should rather consider them as the effect than as the cause of disease; for it is a well known fact, that almost all animals, when become more or less sickly or unhealthy, or approaching to a state of disease, are found to generate *animalculæ* in various parts of the interior of their bodies, and which are seen in the form of intestinal worms, &c.

As I have, I am afraid, departed too far from the subject, I shall now again return to the function of absorption by the extreme radicles of the mesenteric veins. Dr. Copland, in his Appendix to the second edition of Richerand's Physiology, 1829, in treating on the function of absorption from the digestive canal, states, "that from the experiments of Tiedemann and Gmelin on absorption, the lacteals take up the digested and dissolved por-

tions of alimentary substances, and convey them, as chyle, through the thoracic duct to the blood vessels; but as odoriferous, colouring, and some saline substances are not absorbed by them, and yet are found in the blood of the vena portæ, and in secreted fluids, it necessarily follows, that there must be some other way than the thoracic duct by which they pass into the blood.

"The following are the chief suppositions which have been offered in explanation of the facts. Either all the lacteals do not enter the thoracic duct, and a part of them join the veins which form the vena portæ, and thus transmit their contents into the blood of the vena portæ; or substances pass directly from the stomach and intestinal canal into the veins; or, finally, both of these suppositions may be true.

These physiologists found that quicksilver, injected into the absorbents of the intestinal canal, easily reached the mesenteric veins and the vena portæ, and this communication was found to take place in the mesenteric glands. By means of this communication, they explain the appearance of *streaks of a substance like chyle*, which is perceived in the blood of the vena portæ after taking food; a fact which has been frequently observed by other anatomists."

In reply to the suppositions which have been advanced by these physiologists, and which are,

" that either all the lacteal vessels do not enter the thoracic duct, and that a part of them join the veins which form the vena portæ, and further, that the communication of the lacteal vessels with the mesenteric veins and vena portæ has been found to take place through the glands of the mesentery by the injection of quicksilver into the lacteals of the intestinal canal," I have only to add, that from my own examinations and injections, as also those of the first-rate anatomists in this country, no communication whatever of this kind has yet been found to occur.

And as regards " the appearance of streaks of a substance like chyle being found in the blood of the vena portæ after taking food," I have only to observe, that a similar appearance of the blood taken from *the arm* of the human subject has also very frequently been observed;* of the real nature and

* " I also mentioned that Professor Mekel informed us, that he had seen white lymph in the same veins. I have frequently seen this appearance in the veins of the intestines; what this is owing to I do not know. It cannot be absorption of chyle from the cavity of the intestines, for then the lacteals would also be found to contain the same coloured fluid. But, on every occasion, where I have seen this appearance in the veins, the lacteals were constantly empty. When blood has been taken from veins of the arm, the serum has been found frequently white as milk. It has continued in this state for months, in the same patient, and at last recovered its natural colour, without our being able to assign any good reason for the *one change or the other.*"—*Cruikshank on the Absorbent Vessels*, p. 25.

cause of both of which I shall endeavour to give a full explanation when I come to treat of the *blood*, *lymph*, and *chyle*, and the varied changes and appearances which these present during their course through the whole circulatory system.

Dr. Copland further observes, that from these and some other experiments, "it was found, that on examining the blood taken from a branch of the mesenteric vein of a dog, to which sulphuro-prussiate of potass had been given, no streaks of chyle were present, but the saline matter was perceived. From this and other experiments, they conclude that the veins of the intestines appear particularly to absorb heterogeneous substances, such as those already particularized, (alcohol, gamboge, indigo, &c.,) whilst the lacteals take up nutritious matter; and, consequently, that substances taken into the digestive canal may pass into the mass of blood, first, through the lacteal absorbents and thoracic duct; secondly, through absorbents which are united with veins in the mesenteric glands; thirdly, through the radicles, or the commencement of the mesenteric veins, which ultimately form the vena portæ."

The *first* and *third* of these positions have already been fully treated on; and I should consider that the various facts, which have been brought forward by modern physiologists, are quite sufficient to convince us that the *principal function* of

the *lacteal vessels* is to *take up*, in the *form of chyle*, the more solid, digested, or dissolved portions of the nutritive substances admitted into the alimentary canal; and, without these vessels having any communication whatever with the mesenteric veins or liver, they are enabled to convey their contents to the receptaculum chyli and thoracic duct.

On the other hand, as far as regards those fluids which do not undergo so true an assimilated process, and in which various heterogeneous substances are occasionally introduced, I have already stated, that I have not the least doubt that they are absorbed by the extreme radicles of the mesenteric veins, and afterwards pass to the vena portarum, and through the liver, as already stated.

But to contend, as is stated in the second position, that the lacteal absorbents communicate with the mesenteric veins through the lymphatic glands, and that by such means a part of the chyle is enabled to pass too, and ultimately circulate with the venous blood *through the liver*, while, on the other hand, the other portion of the chyle is supposed to pass as in the common acknowledged way to the receptaculum chyli and thoracic duct, is, in my idea, to maintain that which is unsupported by any proofs, either anatomical or physiological.

If we minutely examine the structure of these lymphatic glands, we find that the *very small veins*

already spoken of, and which are those through which *this communication* is supposed to take place, instead of, as imagined, having any thing whatever to do with the lacteal vessels, do, in place thereof, take their extreme *origin from*, and are only the corresponding red veins belonging to, those small arterial branches which are given off from the trunks of the mesenteric arteries at the root of the mesentery; and which arteries, no doubt, are for the purpose of supplying not only the substance of the glands, but also the *immediate surrounding portions of the mesentery* with blood for their support.

These arteries, therefore, like the arterial branches in other parts of the body, are found to terminate in two orders of vessels; that is, first, in veins which convey back the red blood toward the centre of the circulation, and which are those just mentioned; and, secondly, by their extreme terminations in the minute radicles of the *transparent* or *white veins*, that are more commonly called lymphatic vessels; and which lymphatic vessels are proved to be the second *class of lacteals*. I have proved this fact some time ago myself, (see *Lancet*,) and I am happy to find from a statement of Boerhaave's, * that the celebrated Cowper also, from minutely injecting these parts with quicksilver, likewise came to the same conclusion.

The second class of lacteals, therefore, not only

* See his Lectures, p. 346.

take their immediate origin from the extreme branches of the *first*, in the mesenteric glands, like the *branches of two trees opposed to each other*, but also from the extreme branches, or the transparent serous termination of the arteries belonging to the lymphatic glands : which last communication has also been proved by that which has already been so often stated, relative to the second class of lacteals receiving red blood from the arterial ; in which case both sets of vessels are found to have become very considerably enlarged in their size, so that the latter (the lacteals) assume the same character as the real veins.

Hence, then, it appears to me, that the second class of lacteal vessels, from having *different origins*, must, of course, be capable of performing a variety of different functions also ; and from common observation, as well as from the anatomical structure of the parts in which these vessels are situated, (for I beg to observe, that, as well as being lacteals, they are also the only lymphatic absorbents found in these parts), I consider that they are capable of performing four distinct functions : first, that of receiving the *chyle*, or *new white blood*, from the extreme branches of the *first class* of lacteal vessels, and probably from the *cells* of the mesenteric glands, in which I have some idea that the *first class* also *terminate*, and from which the *second class* likewise *take their origin*. Secondly, that of receiving and

of returning back towards the centre of the circulation the transparent lymph, or *white venous* blood, from the extreme or serous branches of the arteries of the lymphatic glands. Thirdly, that of also taking their immediate origin from and of absorbing a portion of the surrounding peritoneal secretion. And, fourthly, as before stated, that, in those extraordinary cases where the function of digestion suddenly becomes suspended, of performing an almost rapid absorption of the adipose substance belonging to those parts, and under such circumstances these vessels becoming so much increased in size as to assume the same character as the real veins.

Both the second and third of these positions will serve to explain that which has been so repeatedly advanced by different authors, respecting the character of the chyle being changed during the passage of this fluid through the mesenteric glands; for, on examination, it is found to have become of a *more coagulable nature* in the second *class of lacteal* vessels, than it was in the *first class*, or prior to its entering into the lymphatic glands; and hence these glands have been considered as designed to effect some very important change in the chyle during the passage of this fluid through them. The reason of the chyle becoming of a more coagulable nature in these glands now appears to me very evident; for, in traversing these parts, the

chyle not only receives a mixture of the absorbed peritoneal excretion, but also a portion of transparent lymph, or *white blood*, from the extreme branches of the arterial vessels of these lymphatic glands ; and if this explanation is just, the chyle ought gradually to become of a *more coagulable* or real blood-like nature as it unites with *the lymph*, or the *more real assimilated white venous* blood in the receptaculum chyli and thoracic duct; and *so it does*, from an admixture of the lymphatic fluid, until it ultimately reaches the right side of the heart, there to unite with the red venous blood from all the various parts of the body, and in combination with it to pass through the lungs, in order, by the union of the oxygen of atmospheric air, to become still more highly assimilated, and of a bright scarlet colour, commonly called arterial blood.